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CASE OF EXTRA-UTERINE FŒTATION.

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Read before the Obstetrical Society of Boston.

THE following case of extra-uterine fœtation, occurring in the person of Mrs. H. C., resident of Gloucester in this State, has so many points of interest that I beg leave to report it.

Mrs. C., æt. 32 years, had been married about nine years and was the mother of two living and healthy daughters, aged, respectively, seven and eight years. Had aborted with a three months' fœtus two years subsequent to the birth of the last child. Health good, thereafter, and menses regular up to April 4th to 10th, 1871. Menses did not appear at their regular period in May, but did occur twice during that month; also, in June and July, at irregular intervals of one and two weeks, lasting from one to four days at each occurrence. Had nausea and vomiting about the last of May, giving first suspicions of pregnancy. Menstrual discharge entirely suspended about the last of July. Near the termination of the last menstrual flow, excruciating pains were experienced in lower part of abdomen, described as "violent cramps," called "colic," by the physician in attendance; lasted several hours; were very prostrating, and relieved only by opiates. Was rarely free from pain and very great discomfort after the attack above-mentioned. Experienced greater soreness of bowels and more obstinate constipation than ever before, when pregnant.

Fœtal life was first felt about the last of August. From this time, gestation went on, without any occurrence of special note, till January 6th, 1872, which was about the date of expected labor. At this date, slight pains were felt in back and lower part of abdomen. Speedy delivery was anticipated. Pain, however, did not become more severe, and lasted only a few hours.

January 12th.—A rapid and vigorous movement of the fœtus occurred on this day, lasting but a few minutes; and, thereafter, all motion entirely ceased. Slight uterine discharge of a dark bloody character was noticed, also, at this date.

There being no recurrence of pain, or symptoms of labor, and no motion of fœtus apparent for more than a week, anxiety was felt, and

a messenger despatched to consult me. It was suggested that a mistake had been made in the "reckoning"—that a few days might yet be wanting to complete the full period of gestation. Patience was, therefore, recommended.

January 30, 1872.—Another week had passed without any change apparent. I was called and found Mrs. C. about house and quite comfortable. Expressing doubt as to the accuracy of her "reckoning," I was assured there could be no mistake, for the period of "quickening," which was specially noted by her, was just 4 1-2 months from the day she recovered from her last regular menses, April 10, 1871, and that the remaining 4 1-2 months should have ended somewhere between the 10th and 20th of the present month, January, 1872.

Palpation of abdomen gave a large, rounded tumor, centrally located, reaching from the pubes nearly to the ensiform cartilage, and occupying the whole abdominal cavity; about the size of the uterus, generally, when containing a fœtus fully developed; somewhat flaccid and less resistant on pressure than the uterus containing a living fœtus. The stethoscope gave no fœtal sounds. Digital examination, per vaginam, gave an os uteri quite constricted and somewhat angular in the direction of its opening, a cervix not obliterated, but, on the contrary, of the usual length and size in the unimpregnated state. Passing the finger above the posterior lip into the "cul de sac," a large, somewhat rounded surface was perceptible to touch, apparently connected with and diverging from the cervix uteri. Sudden pressure made upon this was felt by the hand upon the abdomen, moving the tumor beneath it very sensibly.

Notwithstanding the peculiar, unimpregnated feel of the os and cervix uteri, unaided by the sound, I dared not declare the case other than uterine pregnancy, and, consequently, suggested that it might yet terminate favorably.

February 19th.—Visited Mrs. C. again. Found her attending to her domestic affairs, and hopeful, though in a dilemma. No change, except slight diminution in size of abdomen. No symptoms of labor, whatever, since my last visit. Urged to do something. In the absence of all signs of fœtal life, I prescribed ergot.

March 14th.—Found abdomen a little more shrunken—tumor not rising to ensiform cartilage by more than one and a half inches. Had had no labor pains, though ergot had been taken freely.

Labor had been so long missed, and no signs of fœtal life apparent for more than eight weeks, exploration of the uterine cavity was now attempted. Sponge tents were employed, but distance from patient made it impossible to insert more than one in the 24 hours, consequently, success was small—the external os and cervix yielding only partially—the internal os still remaining undilated.

March 16th.—Quite dissatisfied with the progress made, and doubtful of my diagnosis, I determined to pass the sound at once,

and explore. The sound entered, but passed the cervix and internal os with difficulty, the canal being unusually uneven and obstructed. Its further progress up the cavity was not free, but it seemed to impinge upon, and then pass by, something solid and impacted. It entered 2 1-2 inches, as per measure, on one of Simpson's sounds.

March 17th.—Patient passed a restless night. Had chills, headache and vomiting; pain in right iliac region, with great soreness upon pressure. Anodyne fomentations were ordered to bowels; opiate enemata; febrifuge mixtures; opium and calomel.

March 18th.—Pain and soreness more diffused, passing over to left iliac region; some nausea and vomiting; loss of appetite; frequent pulse and considerable heat. Tympanites was present, accompanied by those peculiar "*quadrupedante*" gurgling sounds so suggestive of peritoneal inflammation.

March 21st.—At this juncture, Dr. A. D. Sinclair, of Boston, was called in counsel. Pain and sensitiveness of abdomen had so far abated, that it was found possible to make a fair examination of patient, though not so complete as might have been desirable. The central position of the tumor in the abdominal cavity, the uniformity of its outline, the projecting points on pressure, the impulse communicated to it by sudden pressure made upon the os uteri, taken in connection with the general history of the case, led to the following result of consultation, viz.: That this was, undoubtedly, a case of pregnancy, and, probably, uterine; that the fœtus was dead; and that nature might yet prove competent to its removal. Patient delay was, therefore, advised.

A few days subsequent to the last date given above, symptoms of inflammation having nearly subsided, a severe and persistent diarrhœa set in. The size of the abdomen was rapidly reduced, and general emaciation and loss of strength became apparent. Nothing peculiar in the discharges was reported, except that sometimes they were a little bloody and slimy, till they had continued nearly three weeks, when, upon inquiry, they were described as very singular in appearance and singularly offensive. Upon inspection made April 24th, more than three months after time of expected labor, portions of the soft parts of a decomposed fœtus were discovered by me, passed, per rectum, by patient. Among them was a piece of intestine, ten inches in length; hair and shreddy portions of scalp and other dermoid texture; also, the body of a small vertebra. Two days subsequently, other bodies of vertebræ and an orbital piece of bone had escaped. The next day the left tibia was expelled, and, finally, a few days after, the left lower maxilla.

The sequel will show that nearly all of the soft parts and many of the smaller bones of a fœtus must have passed the rectum previously to my first examination of the discharges, the stupidity of the nurse in attendance not being equal to their recognition.

These portions of a fœtus discharged, per rectum, pointed to a

diagnosis—rather, were leading to a prognosis. Digital examination was, therefore, made to ascertain, if possible, the orifice through which they were protruded into the rectum, and the direction of the opening. The lower margin of the orifice only could be reached, and that was found upon the anterior wall of the bowel. The feeble condition of the patient seemed not to warrant further disturbance; so the question as to which variety of extra-uterine fœtation this case belonged remained still unsettled.

From the date of the discharge of the last piece of bone, May 4th to May 20th, the day of her death, the patient suffered but little severe pain. Soreness was always present within the abdominal parietes. Loss of appetite and nausea were constant, and, notwithstanding the diarrhœa was nearly suspended and the most sustaining treatment pushed, she gradually failed, and, finally, gangrenous inflammation setting in, she sank into an unconscious state three days prior to death.

Autopsy, 18 *hours after death.*—Cadaver much emaciated; rigor mortis slight; skin of a jaundiced hue, except over lower abdomen which was livid in color; marked fulness of hypogastric region, most marked on the right of median line. The point of special interest being in this region, attention was directed to its examination. The integuments having been carefully removed, a dark and very thin muscular filament was found, covering a hard and somewhat irregular mass. Dissection seemed impossible, for the first touch of the knife penetrated the covering. Upon dilating the opening, a large cavity or sac presented, filled with fœtal bones and a dark pultaceous substance of the consistence and appearance of soft, dark putty. The large cranial bones formed the walls of this mass. The other bones and the soft substance filled the space between them. This mass was carefully removed and the cavity examined in situ, and from above. Its position was between the pubes and umbilicus, the superior part reaching the umbilicus, the inferior part extending downwards into the pelvis, and the anterior portion projecting furthest on the right side of the median line. The shape of this cavity was oblong rather than round—its longer diameter, 6 1-2 inches, extending downwards in the direction of the pelvic cavity—the shorter diameter, 5 1-2 inches, being on a line nearly parallel with the median line. The inner walls of this cavity or sac were ragged and sloughy, greenish brown, or black in color, and very fetid. No placenta or placental attachment was evident. Inferiorly and inclined to the right side and bottom of the cavity, lay the uterus, twisted upon its axis and retroverted to the right. Inferiorly and to left and a little posteriorly, a large, sloughy opening presented, surrounded by soft tissue. This opening communicated with the rectum. The wall of this sac was very thick and dense where connected with the uterus and the broad ligaments, and cut like uterine tissue; but anteriorly and superiorly, it was found agglutinated to the wall of the abdomen, to the omentum and the intestines, and in

a gangrenous, sloughy condition. The bladder was slightly adherent to the sac, otherwise it was in its normal condition. The uterus, rectum and that portion of the sac not decomposed were removed for further examination. The os, cervix and the whole anterior half of the uterus to extreme point of fundus were found in nearly normal condition—the peritoneal surface only showing effects of inflammation. The posterior half of the uterus, which formed part of the base of the sac, was not in normal condition, its surface having undergone absorption and partial change of structure. This surface was continuous with the inner wall of the sac, which wall of sac took on the appearance of homogeneous tissue with the uterus. The right ovary and Fallopian tube could be identified, but were much changed in position and condition from inflammation of tissues. The left ovary could not be identified, and the left Fallopian tube could not be well isolated, owing to agglutination to the walls of the sac.

The cavity of the uterus was pervious, and, apparently, normal. The anterior wall was of normal thickness; the posterior wall, not so thick as the anterior wall, as indicated above. The rectum was in normal position, with reference to the cervix uteri, but above the cervix it was to the left and attached, by adhesive inflammation, to the posterior basal wall of the sac. Along the line of this attachment, an oblong, sloughy opening communicated, directly, with the cavity of the sac in which the fœtus lay. This opening was 1 3-4 inches in length, and from 1 inch to 1 1-2 inches in width—the lower margin being opposite to and on a level with the os uteri.

Inspection of the mass removed from the sac revealed nearly all of the larger bones of a fœtus, except those already mentioned as having escaped per rectum. Most of the smaller bones, carpal, tarsal, phalangeal and vertebral were missing. Such as were found, with an exception or two, I am able to produce. The soft material was made up, in most part, of the disintegrated and decomposed soft parts of the fœtus that had not escaped per rectum. Besides the bones, I have the satisfaction of being able to produce the uterus itself, with the sac also, so much of it, at least, as could be preserved; enough, however, I trust, to verify my statements and my conclusions.

In regard to the size of the fœtus, in evidence of its age and maturity, I give the length of the shaft of the femur, 2 3-5 inches; length of shaft of tibia, 2 1-4 inches. The epiphyses, together with their cartilages, were destroyed or lost from all the bones when first discovered and removed from the sac. Upon comparison of the femur and tibia and other bones of this fœtus with those of a fully grown nine months' fœtus in the Warren Museum, they were found quite equal to them, both in size and length, well establishing the fact that they belonged to a fully developed nine months' fœtus.

The pathology of this case is intensely interesting. Its consideration, naturally, gives rise to the following questions, viz.: What

accident occurred to this ovum, that it should have been the cause of so much mischief? Where was it at the time of the accident, and what were its location and relations, subsequently? What connection had it, if any, with the uterus or generative system? Finally, when the perfect development of the embryo existence had been attained, and that existence proved a failure, what were the resources of nature, in self-preservation, for the removal of the offending body that threatened the integrity of her organization?

The length of this paper, already, will allow me to give these questions only a passing notice; I shall then be happy to leave them to be discussed, in their nicer points, by those better able to do it than myself.

The Graafian vesicle had burst, and the ovum, impregnated or ready for impregnation, had left, or was prepared to leave its primitive nidus, embraced by the fimbriated extremity of the left Fallopian tube. Had it entered the tube, impregnation was possible to it there. Once in the tube, its natural course was to have been transmitted along the lining membrane of the tube into the cavity of the uterus. But we are certain it never reached that cavity, else we should have had normal, uterine gestation, which facts controvert. Neither could it have entered and remained in the tube, for, in that case, we should have had tubal gestation, which, I think, cannot be satisfactorily demonstrated upon the specimen; for, though the track of the tube is sufficiently evident, there is no apparent dilatation of it sufficient to answer that supposition. Could it have become entangled in the interspaces of the uterine tissue, after having been transmitted along the tube to a point between the external wall and the termination of the tube at the internal or cavity wall of the uterus? I must confess this to have been the view I took of the matter when I first looked at the specimen, so evident was the lesion of the uterine tissue. But since reviewing the pathological appearances of the specimen with Dr. R. H. Fitz, of Boston, I fail to be satisfied with that solution of the difficulty. Could it have occurred in the ovary? This point may need further elucidation. But, as the ovary cannot be well identified or isolated from the walls of the sac, and, of course, no point of lesion in that body be detected, I cannot speak with any precision. The fact of a fully developed nine months' fœtus being found, would seem rather to antagonize, if not forbid this view.

The most correct, and the least difficult answer to the question of accident would seem to be this, viz.: That, being impregnated, the ovum dropped—from some physical or emotional cause—from the Fallopian tubal extremity into the peritoneal cavity of the abdomen, at the left side of the uterus; that, lying there and consequent upon impregnation, its membrana decidua was thrown out, probably attaching itself to the posterior surface of the uterus as well as to the peritoneal cavity in which it lay; that, as development went on and larger space was required, the uterus was pressed towards the right side of the pel-

vis, raised or anteverted and then twisted upon its axis, and, finally, almost completely retroverted, thus exposing its posterior surface to pressure from above, as the encysted embryo developed and rose up into the abdominal cavity; and, that thus, from pressure combined with other influences, the uterine surface was so acted upon as to have occasioned absorption and loss of its tissue, and caused its conformity to the other internal wall of the sac.

The question of dependence upon the uterus as the medium of the embryo development cannot be satisfactorily answered upon the specimen. I cannot see any reason, however, to contradict the probability of the attachment of the placenta upon the posterior wall of the uterus.

The last question—the resources of nature for the removal of the dead fœtus from its unnatural and offending position—is satisfactorily answered by the specimen. Gravitation and attrition, at the base of the sac, acting upon the most yielding tissue, the rectum, through the aid of inflammatory and absorbent action, accomplished all that could be done for the liberation of this "failure of nature." This process was partially successful, and, probably, would have been fully successful, had there not been a limit to the accommodating powers of nature in the human organization.

The literature of this class of cases, though exceedingly interesting, I have not time to enter upon. Allow me, therefore, simply to refer for some valuable information to this JOURNAL, vols. 53, 55, 56 and 61; also, vols. 6 and 9, new series. In these volumes I find most of those cases noticed, which have occurred in this State, and many others of interest occurring abroad.

Gloucester, June 2, 1873.

NOTES ON THE PNEUMOGASTRIC.

By J. F. ALLEYNE ADAMS, M.D., Pittsfield.

Read before the Massachusetts Medical Society, June 3d, 1873.

IN presenting to the Society a brief paper upon the functions of the pneumogastric nerve, and their bearing upon the etiology and treatment of certain forms of disease, a few remarks will be offered, by way of introduction, upon the anatomical relations of the pneumogastric to the other cranial nerves.

By the classification of Soemmering, the pneumogastrics constitute the *tenth* pair, and by that of Willis a portion of the *eighth*. The more scientific classification of Müller calls the pneumogastric, with the glosso-pharyngeal and spinal accessory, the *second* pair of cranial nerves, the third, fourth, fifth and sixth of Willis, with the portio dura of the seventh, being grouped together to form the first, while the hypoglossal is the third. This arrangement is more in accordance with the teachings of philosophical anatomy, inasmuch as it harmonizes the nerves with the vertebrate type of the skull, giving

one pair for each of the three cranial segments. It leaves out of the question the three nerves of special sense, viz., olfactory, optic and auditory, which are demonstrated by Oken to have no true nerve structure, but to be mere outgrowths or appendages of the brain. But Müller's classification does not complete the homology between the cranial and spinal nerves, for the third pair has no sensitive root, and the distribution of both the second and third is greatly at variance with the anatomical relations of the vertebræ to which they respectively belong.

Dalton* proposes a fourth classification, which is identical with Müller's, excepting that the hypoglossal is put with the glosso-pharyngeal to form the second, while the pneumogastric and spinal accessory constitute the third. Although suggested by him on purely physiological grounds, this arrangement commends itself, as well, from an anatomical point of view; and there are strong arguments in favor of its adoption as the true classification of the cranial nerves.

These arguments may, for convenience, be arranged under the three following heads:—

1st. Origin. 2d. Distribution. 3d. Function.

1st.—*Origin.*—All of the cranial nerves, excepting the three nerves of special sense mentioned above, have their deep origin in the medulla oblongata, and are arranged in the following order:—from its upper portion arise, in close proximity, the trigeminus, the facial, motor oculi, abducens and patheticus; then comes the glosso-pharyngeal, next below which is the deep origin of the hypoglossus; below this is the vagus, and, lastly, the accessorius. Thus we see that the hypoglossus, although having its origin on the same level as the vagus, and anterior to it, really arises above that nerve, and close to the origin of the glosso-pharyngeus. Arguing from the deep origin, therefore, there is no propriety in calling the hypoglossus the last of the cranial nerves.

2d.—*Distribution.*—The several nerves grouped together as the first pair are distributed chiefly to the frontal segment, including its inferior or hæmal arch, the inferior maxilla. The second, or parietal segment, has for its hæmal arch the styloid processes and hyoid bone; and we find the glosso-pharyngeus and hypoglossus distributed to the tongue and pharynx, parts intimately connected with these bones. The third, or occipital segment, by an adaptation to special ends, has its hæmal arch separated from it, to form the scapular arch; and, accordingly, the third pair of nerves undergoes a similar deviation, the pneumogastric and accessorius being distributed to the whole respiratory apparatus, the heart and chylo-poietic viscera, while the spinal portion of the spinal accessory goes to the trapezius and sterno-mastoid muscles, which form the chief connecting links between the occipital vertebra and its hæmal arch.

3d.—*Function.*—The present classification makes the analogy of

* Dalton's Human Physiology, p. 432.

the cranial to the spinal nerves the more striking, in that it gives to each pair a sensitive and a motor root. Thus, the first pair has for its sensitive root the trigeminus, and for its motor the facial, of which the three motor nerves of the eye and the motor root of the fifth pair are mere accessories. The second pair has the glosso-pharyngeal for its sensitive, and the hypoglossus for its motor root, while the sensitive root of the third pair is the vagus, and the motor the spinal accessory.

Of the arguments that may be adduced in antagonism to this classification, the strongest is, that, in frogs, the hypoglossus is placed *below* the vagus, and becomes the first of the spinal nerves; although the deep origins of the two nerves, even in this case, have been shown by Vogt to be close together.* Another objection is, that the hypoglossus leaves the skull by the anterior condyloid foramen, which is posterior to the foramen lacerum posterius, which transmits the pneumogastric. Therefore, although both of these objections might possibly be ruled out, the arrangement is not to be accepted without due caution.

The pneumogastric arises, superficially, by eight or ten filaments, from the lateral portion of the medulla oblongata, behind the olivary body, between the roots of the glosso-pharyngeal and accessorius. Its deep origin is from a grey nucleus in the lower portion of the floor of the fourth ventricle. The nerve leaves the cranial cavity by the foramen lacerum posterius, within which is formed the ganglion of the root, the ganglion of the trunk being external to the cranium. The root of the pneumogastric is strictly sensitive, but its trunk is both sensitive and motor. Its motor filaments are derived chiefly from the accessorius, which unites with it just below the ganglion of the trunk, besides giving off some filaments to the ganglion of the root. It likewise anastomoses at the ganglion of the root with the facial and hypoglossal, and, at that of the trunk, with the hypoglossal and first two cervical nerves. Both ganglia are intimately connected by numerous filaments with the superior cervical ganglia of the sympathetic, while its trunk receives branches from the middle and inferior cervical and upper dorsal ganglia.

Thus constituted and reinforced at its origin, the par vagum is prepared for a distribution more extensive and functions more varied and remarkable than any other pair of nerves. Its functions may be briefly stated, as follows:—Its *sensitive* filaments are distributed, through the auricular, to the external auditory meatus and membrana tympani, through the superior and inferior laryngeal, anterior and posterior pulmonary, to the whole respiratory tract, through the cervical and thoracic cardiac to the heart, and through the pharyngeal, œsophageal and abdominal branches to the digestive tract.

Its *motor* filaments are distributed, through the pharyngeal and inferior laryngeal branches to the pharynx, through the recurrent la-

* J. Wyman. Nervous System of Rana Pipiens. Washington, 1853.

ryngeal to all of the muscles of the larynx except the crico-thyroid, through the superior laryngeal to the crico-thyroid, through the recurrent laryngeal and pulmonary branches to the tracheal and bronchial muscular coats; and, through the œsophageal, gastric and intestinal branches to the muscular coats of the œsophagus, stomach, and the whole of the small intestine. Until the discovery of the intestinal branches by Kollman, in 1860, the abdominal distribution of the vagus was supposed to stop at the pylorus.

The pneumogastric sends *secretory* fibres to the gastric and intestinal mucous membrane.

Its hepatic branches preside over the glycogenic function of the liver.

It also sends inhibitory or arresting fibres to the heart.

It transmits accelerating and inhibiting fibres to the centre of respiration.

It sends accelerating and inhibitory fibres to the vaso-motor centre, the latter being contained in the depressor nerve of Cym and Ludwig.

Without attempting to go over all of the ground here indicated, this paper will confine itself to the more recent discoveries concerning the relations of the pneumogastric to the heart, the lungs and the stomach.

Influence of the Pneumogastric upon the Heart.—Although the vagus supplies to the heart both centripetal and centrifugal filaments, its action upon this organ is not that of an ordinary motor nerve. On the contrary, it exerts an arresting or regulating influence. The rhythmical contractions of the heart are induced by an excitation derived from the cardiac ganglia of the sympathetic system, beautifully described and figured by Pettigrew, in the May number of the *Edinburgh Medical Journal* for the present year.* This system bears to the heart the same relation that the main-spring does to a watch, while the pneumogastric answers, in some sort, to the escapement. This influence is commonly known as the *inhibitory* action of the pneumogastric.

The subject of inhibition was first introduced to the profession in 1846 by the brothers Weber, who discovered that galvanization of the pneumogastrics in the neck rendered the action of the heart slow; and, if the current were sufficiently powerful, arrested the heart in diastole. Since then, numerous experiments have demonstrated the accuracy of their observations, and have added largely to our knowledge in this direction. The result of these experiments may be stated as follows:†—

If one of the pneumogastrics be divided in the neck, the frequency of the heart's pulsations is slightly increased and the cardiac pressure slightly diminished.

* James Bell Pettigrew. On the Physiology of the Circulation. Edinburgh Medical Journal, January to May, 1873.

† Flint. The Physiology of Man. Vol. IV. Nervous System. 1872.

If both pneumogastrics are divided, the frequency of the pulsations is greatly increased, at least doubled, while the force is greatly diminished.

Galvanization of the peripheral end of the divided nerve retards or arrests the heart's action; but galvanization of the central ends produces no such effect, showing that the inhibitory influence is a centrifugal one.

If an animal be placed under the influence of woorara poison, which paralyzes motor nerves without affecting sensitive nerves or muscular irritability, and the pulsations of the heart be kept up by artificial respiration, galvanization of the pneumogastrics has no effect upon the heart's pulsations; whence we may conclude that the inhibitory power is located in motor filaments. This experiment has been tried by Bernard and verified by Flint.

The experiment has been performed by Traube of injecting digitalis into the veins of a dog with the pneumogastrics divided, without affecting the heart's pulsations; although, when this was done with the nerves intact, the number of beats was, in an hour, reduced to one-fourth of the normal number. This indicates that the sedative action of digitalis upon the heart is due to an irritation of the vagus, transmitted from the brain.

Irritation of the medulla oblongata produces the inhibitory action; but the irritation is conveyed, not by the root of the vagus, but, as has been demonstrated by Waller, by the accessorius.

Recently, it has been discovered that the right vagus possesses a more powerful inhibitory power than the left. "Dr. Massin states that, while the movements of the heart are stopped by a very powerful excitation of the left vagus, it is always possible, by sharply striking the heart with the finger, to reproduce the rhythmical movements; while this mechanical cause of action remains ineffectual when applied to the heart stopped by strong galvanization of the right vagus."*

The inhibitory influence upon the heart is also exerted in various reflex ways. Brown-Séquard† has found, in rabbits and guinea-pigs, that an irritation of the nostrils by carbonic acid or chloroform, or even by a sudden and gentle touch, will cause the heart to stop completely, or, at least, notably diminish its force and speed. Rutherford states that, if any irritating vapor be brought before the nose of a rabbit, it closes its nostrils and ceases to breathe, often for thirty or forty seconds. Within three seconds after the cessation of respiration, the heart comes almost to a stand-still, and continues to beat very slowly until respiration is re-established. Rutherford believes that "this arrest of the heart is due to stimulation of the inferior cardiac branch of the vagus by the asphyxiated condition of

* Brown-Séquard. On the Sudden or Rapid Arrest of Certain Normal or Morbid Phenomena. Archives of Sci. and Pract. Med., Jan., 1873.

† Ibid. Most of the physiological facts in this paper are from Flint or Brown-Séquard.

the blood"; but Brown-Séquard proves conclusively that this is not the case, but that the phenomenon is due to reflex influence.

The cold bath, especially the shower-baths or douche, has, by reflex action, the same inhibitory influence upon the heart. Fainting is even produced in some persons by this means, from complete arrest of the pulsations. Brown-Séquard states that, in Europe, where ice-water is not as habitually used as here, a stoppage of the heart's action is not rare, in the summer season, after the drinking of a large quantity of cold water.

Wounds of the abdominal viscera and blows upon the belly produce fainting or slowing of the heart's action. To Brown-Séquard we are indebted for the discovery that the crushing of the ganglions of the sympathetic in the abdomen stops or diminishes the movements of the heart; but if the spinal cord or vagus is divided transversely, the crushing can be made without any influence upon the heart.

Goltz has demonstrated, in the frog, that the heart can be brought to a stand-still by striking upon the abdomen, and that this paralysis of the heart can be prevented by a simultaneous strong irritation of the cutaneous nerves, which diminishes or abolishes the reflex activity.

It is asserted by Rouget, that the inhibitive action takes place in nerve-cells. He says:—"Wherever the excitation of a nerve is followed by an arrest of movement, nerve-cells are found on the nerve-fibres which transmit the excitation." In this view he is supported by Brown-Séquard, who thus describes the process of arrest:—

1. An irritation starts from a part of the nervous system radiating towards the nerve-cells, the activity of which is to be arrested.

2. A special influence is to be exerted by this excitation on those cells, arresting their activity.

3. The movements, normal or morbid, or other phenomena depending on the activity of those cells, cease at once when that activity ceases.

In the discovery of the *depressor* nerve by Cym and Ludwig, who first published an account of it in 1867, an important addition is made to our knowledge of the pneumogastric in its relation to the heart. This nerve was found in the rabbit, arising by two roots, one from the trunk of the pneumogastric, and the other from its superior laryngeal branch, passing down the neck, by the side of the sympathetic, to the chest, where, joining with sympathetic filaments, it passes to the heart by little branches between the origin of the aorta and the pulmonary artery. This nerve has since been found by Cym in the horse. In the rabbit it has been made the subject of a very complete and satisfactory series of experiments.

When this nerve is divided in the neck, and its *peripheral* end irritated by galvanization, no effect whatever is produced upon the heart or the circulation; but if its *central* end is galvanized, the pressure is diminished in the whole arterial system. This diminution is gradual,

the pressure never reaching its minimum before fifteen pulsations of the heart, and may be reduced to one half the pressure before the irritation was applied. At the same time that the arterial pressure is diminished, the frequency of the heart's pulsations is reduced, *unless both pneumogastrics have previously been divided*, when, although the arterial pressure is reduced as before, the action of the heart is not disturbed. These experiments prove that the action of the depressor is entirely centripetal, that its inhibitory action upon the heart is reflex, through the nerve-centres, the trunk of the pneumogastric, and its remaining cardiac branches, while the reduction in the pressure of blood in the large arteries is independent of the centrifugal fibres of the vagus, and bears no relation to the reduction in the number of cardiac pulsations.

In determining the mechanism whereby the reduction in the arterial pressure is effected, Cym has displayed great ingenuity. Having, by a process of exclusion, eliminated all other possible agencies, he has clearly shown that this influence is exerted by reflex action, through the instrumentality of the splanchnic nerves, the most important vaso-motor nerves in the entire organism. "The irritation of the depressor nerve, after section of the splanchnic nerve," to use the language of Cym, "produced still a diminution in the blood-pressure, but the absolute value of this diminution is much less than it was during the irritation of the depressor nerve, before the section of the splanchnic."

The result of these experiments is to show that irritation of the depressor nerves exerts, through the splanchnic, an inhibitory or paralyzing influence upon the abdominal vaso-motor system; and thus, the resistance to the flow of blood being diminished in the immense vascular system of the abdominal organs, the pressure is correspondingly reduced in the arterial trunks.

(To be continued.)

Progress in Medicine.

REPORT ON PHYSIOLOGY.*

By H. P. Bowditch, M.D.

Animal Heat.

Rosenthal.—Zur Kenntniss der Wärmeregulirung bei den Warmblütigen Thieren. Erlangen. 1872.

Liebermeister.—Zur Lehre von der Wärmeregulirung. Virchow's Archiv, Bd. 52, p. 123.

Roehrig and Zuntz.—Zur Theorie der Wärmeregulation und der Balneotherapie. Pflüger's Archiv, iv. p. 57.

Paalzow.—Ueber den Einfluss der Hautreize auf den Stoffwechsel. Pflüger's Archiv, iv. 492.

* Second semi-annual Report.

VIRCHOW.—Wirkung kalter Bäder und Wärmeregulirung. Virchow's Archiv, Bd. 52, p. 133.

SENATOR.—Untersuchungen über die Wärmebildung und den Stoffwechsel. Reichert und Du Bois Reymond's Archiv, 1872, p. 1.

WINTERNITZ.—Beiträge zur Lehre von der Wärmeregulirung. Virchow's Archiv, 1872, p. 181.

GARROD.—On the Relation of the Temperature of the Air to that of the Body. Journal of Anatomy and Physiology, vol. vi. p. 126.

HORVATH.—Zur Physiologie der thierischen Wärme. Centralblatt für die Med. Wiss., 1872, p. 706.

Zur Lehre vom Winterschafe, ditto. 1872, p. 865.

DRAPER, J. C.—On the Heat produced in the Body and the Effects of Exposure to Cold. American Journal of Science and Arts, December, 1872.

JUERGENSEN.—Die Körperwärme des gesunden Menschen. Leipzig. 1873.

We have, in the above-mentioned pamphlet of Prof. Rosenthal, an excellent *résumé* of the most important recent investigations in this department of physiology, and a clear statement of the present condition of our knowledge of the subject. This work is one of especial value to practising physicians, now that the thermometer has come to play such an important part in diagnosis and prognosis, for we find here a clear statement of the conditions under which alone accurate measurements of animal heat are possible. For a better comprehension of the problem, the author conceives the animal body as divided into three portions:—

I. A central portion, where the temperature is uniform, or nearly so, throughout.

II. An external layer, exposed to the cooling influences of the air.

III. An intermediate zone, where the temperature increases gradually from without inwards. The thickness of this zone differs in different parts of the body, being greatest in the most exposed portions.

To determine the real temperature of the animal body, the thermometer must be passed through the intermediate zone, and the measurements taken in the central portion. This is best accomplished in the rectum, and in order to be sure that the instrument lies in the central portion, it must be introduced slowly and the temperature read in different positions. As long as the mercury continues to rise in proportion to the depth to which the thermometer is introduced, it must be regarded as lying in the intermediate zone; but when a deeper introduction no longer has this effect, the reading of the thermometer may be considered as expressing the absolute temperature of the body. All the other parts of the body, which are used for thermometric purposes, viz., mouth, vagina, axilla, &c., lie in the intermediate zone, and give a temperature not only lower but more variable than that of the rectum. This is especially true of the axilla, whose temperature, being greatly dependent upon the vascularity of the skin, is sometimes found to rise under circumstances which cause that of the rectum to sink.

The constancy of the temperature of a warm-blooded animal is, of course, dependent upon an equilibrium being established between the production and the loss of heat in a given time. Any increase of the production or diminution of the loss of heat will therefore raise the

temperature of the body, and, conversely, any diminution of production or increase of loss will lower it. The regulatory arrangements, by means of which the body is able to maintain the uniformity of its temperature in spite of variations of its surroundings, affect chiefly the loss of heat. Thus, exposure to cold causes contraction of the cutaneous bloodvessels, in consequence of which less warm blood is brought to the surface of the body, and therefore less opportunity offered for loss of heat by radiation. Exposure to heat has, of course, the opposite effect. The power of the body, however, to keep its temperature constant is confined within narrower limits than has been supposed. Rosenthal found, in his experiments on rabbits, that these animals could only preserve their normal temperature when the surrounding media had a temperature between 11° and 32° C. Man, who is able to adapt himself to a greater variety of climates than any animal, owes this power chiefly to his ability to effect voluntary changes in his food and clothing. Even in man, it has been found that a slight increase in the loss of heat, as by sudden removal of the clothes, is sufficient to cause a perceptible lowering of the temperature in the rectum.

There seems to be often a great want of promptness in the working of the regulatory arrangements, for Rosenthal found that, when a rabbit had been exposed for a certain length of time to air of 36° – 40° C., and then removed to air of ordinary temperature, the thermometer in the rectum indicated, for the next two or three days, a diminution of about 1° C. in the temperature of the body. When exposed to a sudden lowering of temperature in the surrounding media, we may "catch cold" in two different ways: firstly, heat enough may be abstracted from the warm surface of the body to cause a dangerous lowering of the temperature of the body; or, secondly (if the cutaneous bloodvessels contract to prevent this loss of heat), a sudden collateral hyperæmia may be produced. The prophylactic effect of cold baths seems to be due to their causing an increased "tonicity" of the cutaneous bloodvessels, so that they do not dilate so much under the influence of a high temperature of the surrounding media. Variations in the activity of the respiration and perspiration also influence the loss of heat from the body.

In addition to these means of regulating the *loss* of heat, are there any arrangements by which the *production* of heat can be varied in accordance with the needs of the body? In other words, does the production of heat in the body vary with the temperature to which the body is exposed?

This a question which, for more than ten years past, has been under discussion in Germany. The most conspicuous of the disputants, with their most recent writings, are given in the above list.

The affirmative side of the question is maintained by Liebermeister and by Roehrig and Zuntz. The facts upon which they rest their opinion are the following:—

I. A cold bath, or exposure of the skin to cold, often causes a temporary rise of temperature in the axilla or mouth. To this Senator and others object that measurements in the axilla do not give the true temperature of the body; that, under the same circumstances, the temperature in the *rectum* speedily falls from the beginning of the experiment; and that the temporary rise in the axilla is due to the constric-

tion of the cutaneous vessels, in consequence of which the blood is not so much cooled in its passage through the limbs. The temperature rises, therefore, in the axilla in the same way as after the application of a tourniquet to the brachial artery.

II. The calorimetrical experiments of Liebermeister seemed to show that, by quietly lying in a bath of 20° – 30° C., not only the loss but also the production of heat is increased; so that in a bath of 20° – 23° C. from three to four times, and in a bath of 30° C. about twice as much heat is produced as by the same individual under ordinary circumstances.

The value of these experiments is disputed by Winternitz and Senator, who point out various defects in the methods adopted. Senator maintains that, in his own more carefully conducted calorimetrical experiments, no increased production of heat under the influence of cold could be detected.

III. Exposure to cold causes an increased absorption of oxygen and elimination of carbonic acid.

Senator admits the correctness of this observation, but concludes, from his own calorimetrical experiments, that heat production and excretion of carbonic acid do not always go hand in hand. He finds, for instance, during digestion, an increase both of carbonic acid excreted and of heat produced, but the production of heat is more increased than the elimination of carbonic acid. On the other hand, he finds that exposure to cold causes no increased production of heat, measured calorimetically, but a decided increase in the elimination of carbonic acid. Hence we must admit that there are processes going on in the body which produce heat, but do not set free carbonic acid; and, on the other hand, that the elimination of carbonic acid does not prove a contemporaneous production of heat.

Roehrig and Zuntz regard the production of heat in consequence of exposure to cold as a reflex phenomenon, dependent upon an irritation of the sensitive nerves of the skin, causing an increased metamorphosis of tissue. This same effect may be produced by irritating the cutaneous nerves in other ways, e. g. by salt water. In this way is explained the invigorating effect of sea-bathing.

(In this connection are to be noted Paalzow's experiments on rabbits, which show that a mustard poultice on the abdomen causes a decided increase in the absorption of oxygen and elimination of carbonic acid.)

Recognizing the important part played by the muscles in the production of heat, Roehrig and Zuntz regard the regulatory process as probably consisting in a weak reflex irritation of the motor nerves, which increases with the difference of temperature between the animal and its surroundings. Under ordinary circumstances, this irritation is not sufficient to cause a visible contraction of the muscles, but under the influence of severe cold we have the phenomenon of shivering, which, on this theory, is only an exaggeration of the normal regulatory process. If this view of the question be correct, we should expect that paralysis of the motor nerves by curare would not only lower the temperature of the body, but would also reduce the regulatory power to a minimum, and this is found to be really the case. On the whole, therefore, although an increased production of heat as the result of exposure to cold cannot be regarded as absolutely demonstrated, yet

it must be admitted that there are many observations which point in this direction. Since we know, by the observations of Brondgeest, that muscular tonicity is a reflex phenomenon, it is reasonable to suppose that this tonicity will be increased by stimulation of the cutaneous nerves. This, of course, will cause an increase of the heat production which always accompanies muscular activity. On the other hand, as Rosenthal observes, the success of the cold-water treatment of fevers would hardly be explicable on the theory that an increased loss is necessarily accompanied by an increased production of heat.

The experiments of Horvath were made on marmots and hedgehogs, with a view of determining some of the characteristic phenomena of hibernation. The temperature in the rectum of a hibernating animal was found to sink as low as 2° C. On awakening from this condition, the temperature rose rapidly to 32° C.; but this rise was preceded (as far as can be judged from the meagre details of the experiments which are given) by an increased rapidity of the respiratory movements. Respiration experiments were made, both before and after waking, showing that the elimination of carbonic acid was four times, and that of water seven times, as great in the waking as in the sleeping condition. Experiments consisting in artificially cooling hibernating animals, showed that their hearts continued to beat and their muscles remained irritable at temperatures much below that at which these phenomena ceased in rabbits.

Draper's experiments had for their object, "to determine the quantity of heat passing off from the surface of the body, by finding how much it would elevate the temperature of a known mass of cool water during a given period of time." It was found that by lying quietly for one hour in a bath of 74° F., enough heat was lost from the body to raise the temperature of the water 2° F. and to lower that of the body, as measured in the mouth and armpit, 1° F. As the volume of water in the bath was seven and one-half cubic feet, and that of the body three cubic feet, it was concluded that enough heat is evolved from the body in one hour to warm the body itself 5° F. Dr. Draper further concludes that "the converse of this may also be considered as true, viz., that after death, the air being at 73° F., enough heat is lost in the course of an hour to cool the body five degrees, at least during the first hour." This, of course, could only be true if the production and loss of heat in the living body were the same as in the corpse, and the conducting power of air the same as that of water. From the figures in Dr. Draper's tables, it can be calculated that in a bath of 74° F. (23.3° C.) the body loses in one hour an amount of heat equal to 231 Calories, i.e. an amount capable of raising the temperature of 231 litres of water 1° C. This loss is not so great as has been observed by Liebermeister and others in baths of similar temperature, and the cause of the difference is perhaps to be found in the greater duration of Dr. Draper's experiments. Thermometers in the mouth and axilla indicated a steady fall of temperature during the bath and for a short time after leaving it. This was accompanied by a diminished rate of respiration and pulse. The continuance of the effect after leaving the bath is not explained by Dr. Draper, but it seems reasonable to suppose that the evaporation of the moisture adherent to the body on leaving the bath may have caused a further reduction of temperature. The fact that the air in the room had a temperature of 90° F. and con-

tained only 53 per cent. of the amount of moisture necessary for its saturation, lends force to this suggestion. The diminished rate of the pulse seems to be due to a direct effect of a lowered temperature on the heart's action as demonstrated by Cyon.* The slow respiration may also be due to the same cause, but it is important to bear in mind that the rate of respiration has no direct connection with the activity of the interchange of gases in the lungs, and also that in counting one's own respiratory movements (as Dr. Draper himself observes), it is "very difficult to avoid influencing them in the act of counting." The most important result of Dr. Draper's experiments is that, when the experimenter in the bath executes continued muscular movements, the temperature of the water around him is not raised any higher than when he remains perfectly at rest. We must therefore conclude that the application of water of 75° F. to the surface of the body reduces to a minimum the function of the skin, considered as a regulator of animal heat, "and the whole duty of exhalation of vapor of water and consequent removal of heat is thrown upon the lungs; hence the increased respiratory actions, and hence also the special tendency of application of cold to the surface to produce inflammations of those organs by increasing the work they are obliged to perform."

Jürgensen publishes in pamphlet form observations previously communicated to the *Deutsches Archiv für klinische Medicin*. According to this observer, the temperature measured in the rectum of a person at rest has a normal daily variation of about 1° C., the highest temperature being in the afternoon, and the lowest between midnight and morning. The taking of food raises the temperature slightly, but not enough to interfere with the regular diurnal variation. The connection of sleep with changes of temperature is unfortunately not spoken of.

Temperature in Right and Left Ventricles.

Koerner.—Beiträge zur Temperatur topographie des Säugethierkörpers. Breslau, 1871.

Heidenhain.—Ueber den Temperaturunterschied des rechten und linken Ventrikels. Pflüger's Archiv, iv. 558.

Bernard.—La chaleur animale. Revue Scientifique, i., 133 et seq.

Stricker and Albert.—Untersuchungen über die Wärmeökonomie des Herzens und der Lungen. Stricker's Medizinische Jahrbücher, 1873, p. 30.

The observations of Bernard and Liebig were for a long time regarded as conclusive evidence that the blood in the left side of the heart is cooler than in the right side, and the conclusion seemed to be justified that the blood in circulating through the lungs is more cooled by coming in contact with the air and by the loss of watery vapor than it is heated by any chemical processes which may have their seat in the lungs. It would, therefore, be expected that the breathing of warm air saturated with moisture would cause a rise of temperature of the arterial blood. The experiments of Lombard,† however, seemed to show that this was not always the case, and further doubts were thrown upon the correctness of the above view by the observations of Jacobson and Bernhardt‡ who, in a series of experiments on rabbits with

* Ueber den Einfluss der Temperaturänderungen auf Zahl, Dauer und Stärke der Herzschläge. Arbeiten a. d. Phys. Austalt zu Leipzig, i. p. 77.

† Archives de Physiologie, 1868, p. 479.

‡ Centralblatt für die med. Wissenschaften, 1868, p. 643.

thermo-electric needles, found that, in the majority of cases, the blood of the left heart was warmer than that of the right.

The question having been thus again opened, a large number of observations were made on dogs by Koerner and Heidenhain, partly with thermometers and partly with thermo-electric apparatus. They found that out of 94 observations only one showed an equality of temperature between the two sides of the heart, while in all the others the difference was in favor of the right side. The authors, however, propose an entirely new explanation of the cause of this difference. According to them thermometric measurements in the ventricles give not only the temperature of the blood contained in them but also that of the muscular walls, for these walls must necessarily at every systole be brought in contact with the bulb of the thermometer or the thermopile. Now the walls of the right ventricle are warmer than those of the left, because they lie in contact with the diaphragm which rests directly upon the warm organs of the abdominal cavity. The real reason then of the higher temperature in the right ventricle is that heat is conducted to its walls from the abdominal organs, while from the left ventricle heat is conducted away by the cooler lungs which surround it. This theory is supported by the observation that the difference of temperature between the two ventricles is increased by keeping the instruments pressed firmly against the muscular walls. Moreover filling the stomach with cold water or placing a bag filled with cold water under the diaphragm was found to diminish this difference, while the use of warm water in a similar way increased it. According to this theory, observations on the comparative temperature in the two ventricles have not the same physiological interest which has been heretofore attributed to them, since the difference of temperature depends chiefly on anatomical relations of organs which may vary in different animals.

Since the publication of Koerner's and Heidenhain's experiments, Bernard has given a course of lectures on animal heat, where he has mapped out very minutely the variations of temperature in the different parts of the animal body. He discusses the above theory and rejects it on the ground that the observations of Hering on a case of ectopia cordis have shown that even in this malformation the temperature of the right ventricle is higher than that of the left. Here, of course, the possibility of the communication of heat between the heart and the neighboring organs is excluded.

Stricker and Albert are also unwilling to accept Heidenhain's explanation, for they find that the difference in temperature between the right and left ventricles is unaffected by opening the abdomen and withdrawing the viscera from contact with the diaphragm. These observers give a very extended series of measurements in all parts of the heart and great vessels, from which they conclude that the muscular walls of the left ventricle are warmer than those of the right, but the blood of the right ventricle warmer than that of the left. This would of course indicate a cooling of the blood in the lungs.

Even if the views of Heidenhain and Koerner should not be fully sustained by subsequent experiments, there is without doubt sufficient truth in them to deprive the question of a great deal of the importance which it was formerly thought to possess.

(To be concluded.)

Boston Medical and Surgical Journal.

Boston: Thursday, July 17, 1873.

THE SUPPRESSION OF QUACKERY.

Of the many difficult questions the solution of which rests with the medical profession, none is more urgent or more important than this. If the law forbade, as it should, under severe penalties, that any one should practise without the degree of a recognized college and the membership of the State Society, there would be nothing to desire; but as this is too much to hope for, for a long time at least, we must consider what is in our power. The *Canada Lancet* proposes to remedy the evil by instructing the people in medical matters, so that they may be able to recognize charlatanism. This plan agrees with the day's hobby of popular science, but we fear it would only increase the evil. The maxim that "a little learning is a dangerous thing," cannot be too often repeated. Every physician can testify that few patients are more troublesome than those who think they know something of medicine. The illustrations used by the *Lancet* in support of its theory do not strike us as happy. It says that "the large audiences which attended the lectures of Prof. Tyndall were pleased and delighted with his experiments and explanations on the subject of light." We have no doubt of it, but we do not believe that five per cent. of his hearers understood the subject, or could to-day give a rational account of his lectures. The second illustration is more unfortunate, as it speaks of the exhibition certain New York professors recently made of themselves in Steinway Hall as something creditable and instructive. The fact is, non-professional people are not able to judge of medical subjects, and it is not possible that they should become so. Nothing that is worth knowing can be mastered without severe study. Amateur medicine is on a level with political strategy, of which our late war afforded such frequent examples. Moreover, it must not be forgotten that the motto of the most pestilent class of quacks is "public instruction," and that it is under this disguise that books "for the young," "for the married," "for the unmarried," containing most pernicious stuff, are peddled through the country. In the same way, a tempting opportunity for attaining notoriety is offered to those who, if restricted to professional means, would see themselves condemned to obscurity. The struggles of the homœopaths to make capital out of the late trial is a case in point.

We think that the remedy is to be found in the direction opposite to that indicated by the *Lancet*. It consists in raising the standard of

medical education, in making a degree something that one may be proud to have earned, in thus forcing the incompetent into the ranks of the irregulars, and in keeping the line between the latter and the profession sharply defined. Such a reform cannot be effected in a day, but it is sure to come if we do our duty. We must expect to be laughed at and sneered at, to be called bigots and fanatics; but in the end, merit will tell, and the dignity of the profession will be recognized. Many forms of error and delusion will fall of themselves, and public sentiment will permit the suppression, with the strong hand, of the viler and more dangerous forms of quackery.

European Child-life in India.—At a recent meeting of the Medical Society of London (*British Medical Journal*, May 3d), Dr. Fayrer read a paper on this subject. He pointed out how rapidly the infant European population was increasing in India, and contrasted the mode of life in that country twenty-five years ago with what it is now. The queston had often been asked: "Can the Anglo-Saxon colonize India; i. e., can the race, unsupported and unrecruited from home, continue to reproduce itself and exist there? Could he, in short, do in India what he has done in America and Australia, people the country and displace or replace the autochthones and his older Aryan brethren who have become acclimatized during an occupation of many centuries?" Dr. Fayrer was of opinion that the data for forming a precise reply to this question did not exist, but his own firm conviction was that it could not be done; and he felt convinced that, had India been colonizable by the European, the position now held by them would be very different from what it is. The European who becomes an item in the fixed population, and who leads an ordinary temperate and correct life, had expectations of living perhaps little below those which he might have had in England. About 1815, an asylum was founded in Calcutta for children whose parents were European, and it is from the reports of this Society, numbering about 129 individuals, varying in age from one to eighteen years, that the following information is gathered. It was observed that the stimulating effects of an almost tropical climate asserted their influence; and, as a rule, the girl of sixteen or seventeen years was two or three years in advance of a girl of that age in a European climate. These children appear to have a great immunity from diseases peculiar to the country as well as from others of a severe kind; thus no cases of cholera, diphtheria, scarlatina, croup, pleurisy, pneumonia, ophthalmia, phthisis, dengue or malarious cachexia, have been known among them for many years. The death rate is about double that in England, as the following table will show:

	England.	India.
Under 5 years	67.58	148.10 per 1000
From 5 to 10 years	8.80	17.73 "
From 10 to 15 years	4.98	11.51 "

The author concluded by saying that it was satisfactory to know that by care and proper training a European child may live, grow, and even thrive, on the plains of Bengal.—*British Medical Journal, May* 3, 1873.

Correspondence.

MESSRS. EDITORS.—A writer in the last number of the *New York Medical Record* discourses somewhat diffusely but decidedly against the practice of supplying *Life Insurance* Companies with "*answers by the family physician*" so unscrupulously demanded by these companies. After saying:—"I have no hesitation in asserting that it is no part of his duty to furnish information to a life insurance company in respect to the health of individuals who may have placed themselves under his professional care. Not only is it no part of his duty as a medical man, but it is virtually a betrayal of the trust and confidence imposed in the unreserved relations between patient and physician"—he adds in closing:—

"It is remarkable that a common professional sentiment did not long ago protest against such an attempt to procure information, on the ground, already mentioned, of infraction of ethics, and disregard of individual obligation and propriety."

Now, Messrs. Editors, such a protest has been made time and again, with some effect undoubtedly, at least in this vicinity, but not with all that can be desired; for not unfrequently these "questions" are thrust upon practitioners in a manner to render consent or refusal very unpleasant. We therefore ask of you to reprint reasons for "declining to answer" which we prepared; and which were, with the hearty endorsement of the editors, published in this JOURNAL a dozen years ago, as follows:—

The family physician, by answering such questions, virtually makes himself an agent of the Insurance Company, and if they are induced, by any inadvertence, error, or design of his, to assume an improper risk, however great may be the ultimate damage to the physician, the Company cannot escape thereby the payment of the amount insured. This is the legal aspect of the case. It is not altered by the applicant's wish to have the questions answered, since he only follows the directions of the Company.

As business men, money seekers merely, the Company ought not to expect that a physician, having no interest in their enterprise, will, for their exclusive benefit, voluntarily seek out and disclose to them, or put on record —the ultimate disposition of which he may know, and over which he can have no control—the constitutional weakness and physical defects of a patient and friend, in whom he may have great interest, and whose prospects he may not wish to disparage, much less to fatally injure. Yet the physician is, in fact, asked to do even thus much, to save a Corporation from the risk of a pecuniary loss.

For their own protection, the Company has, or should have, competent examining and consulting physicians, on whose investigations and judgment they should be willing to rely.

The relation between physician and patient is such, that a public parade of the former's knowledge and opinions, relative to the physical infirmities of the latter, may be accompanied, in almost every instance, with, to say the least, many serious objections. To medical men, these objections need be only alluded to. To such they are sufficiently obvious and obligatory. If they are not so to insurers, it may be from want of experience in that direction; but while they can obtain all needed information through their own examiners, without incurring these objections, they ought to be satisfied therewith.

In a word, Life Insurance Companies have *no right whatever* to make such demands of medical attendants, and it is quite time that the practice be discontinued.

We wish now to add to the reasons in the preceding extracts, that up to this time no good reason has been given why "the family physician" should answer such "questions" propounded by Insurance Companies. Certainly he should not do it for the sake of a fee,—there are some things which should be above, and unreachable by, price.

June, 1873. IATROS.

Medical Miscellany.

DR. HENRY TUCK has been elected one of the physicians to out-patients at the Massachusetts General Hospital.

WE are sorry to learn that Mr. Erichsen has been forced by illness to abandon work for some time to come.

IT is unkind of the *Chicago Medical Examiner* to accuse us of reporting incorrectly the proceedings of the American Medical Association, for we have not noticed them at all.

THE Trustees of Jefferson Medical College, unable to agree upon a successor to Prof. Pancoast, have adjourned the selection to October next.

CHOLERA of a virulent type has broken out in Cawnpore, and in Lucknow smallpox has made its appearance.

RIGID EXAMINATIONS.—Within the last ten months there have been four hundred and forty-three candidates for membership of the Royal College of Surgeons, England. Of this number, one hundred and four have been rejected.

Dr. COLTON thinks this the only dental, medical or surgical journal that does not acknowledge that Dr. Wells discovered anæsthesia. We naturally feel flattered, but fear that a large number of our contemporaries will not permit us to accept the compliment.

WRIGHT'S FORMULA FOR HEADACHE FOLLOWING ALCOHOLIC DEBAUCH.—Take of solution of acetate of ammonia, tincture of bitter orange peel, syrup of bitter orange peel, each 20 parts; water, 500 parts. To be given in repeated tablespoonful doses.—*Revue de Thérap. Med.-Chir.; Canada Medical Record.*

Re-printed for the use of prohibitionists.

HARVARD UNIVERSITY—CHANGES, &C.—Charles B. Porter, now demonstrator, to be assistant instructor in surgery; Clarence J. Blake, M.D., and J. Orne Green, M.D., lecturers on aural surgery; F. B. Greenough, M.D., and E. Wigglesworth, M.D., lecturers on syphilis; James R. Chadwick, M.D., lecturer on the diseases of women; Charles P. Putnam, M.D., lecturer on the diseases of children; James J. Putnam, M.D., lecturer on the application of electricity to nervous diseases.

RUPTURE OF THE BLADDER FROM INJURY; RECOVERY OF THE PATIENT.—Dr. J. Mengus *(Abeille Médicale)* records a case of this accident where the usually fatal result did not ensue.

At 9 o'clock in the morning, a young man, æt. 34, fell on the edge of a wine-press from a considerable height; he immediately felt an acute pain in the pelvic and right hypochondriac region, but was able to walk and get home to bed. He was seen two hours after the accident, and presented the following symptoms:—Pulse very slow and feeble; the right side of the belly swollen, tender, and dull on percussion; he had not passed urine since 4 o'clock in the morning, and could now pass only a few drops. A catheter was passed, which obtained but a few more drops, and a careful examination was made of the bladder, but no rent or opening could be ascertained. The bladder was then injected with warm water. The greater part, but not all, flowed back, tinged with blood. He was treated with opium, camphor, and quinine in pill frequently repeated. On the same day, in the evening, the pain increased; leeches were applied over the belly; he restrained himself from drinking as far as possible, and by the fourth week the symptoms had disappeared. He could retain and urinate as before.—*Dublin Medical Press and Circular.*

ONE profession (says *Punch*) is safe from the invasion of woman. She may enter the army, but it is impossible that she can man the navy.

THE Overseers and Trustees of Bowdoin College have made the following elections for the Medical School of Maine. Dr. E. W. Jenks, Professor of Diseases of Women; Dr. Alfred Mitchell, Professor of Obstetrics and Diseases of Children; Dr. Robert Amory, Professor of Physiology; Dr. T. H. Gerrish, Professor of Materia Medica. The statement in some of the daily papers that Dr. Dwight was elected Professor of Anatomy is not strictly correct. He was *reëlected*, in order to correct certain clerical irregularities.

UNCIVIL JOURNALS.—The *Western Lancet*, of San Francisco, shows its usual good taste in re-publishing our article (occupying twenty-one pages of that journal) on "Rhus Venenata and Rhus Toxicodendron," by Dr. James C. White, with the remark that it is "peculiarly interesting" to practitioners on that coast. No remark is made, however, as to the source from which the article was taken. The February number of "the largest medical monthly in America" contained over fifty-nine pages of material taken from the *New York Medical Journal*, without the slightest acknowledgment—one paper contributed by Dr. Sims constituting nearly half that issue.—*New York Medical Journal.*

NOTES AND QUERIES.

WHY do not booksellers advertise their literary works more freely and frequently in medical journals? Are they not aware that, as a general rule, physicians are the great promoters of reading and study in the various circles of their clients, especially in country places? If they are unable to buy books themselves, they recommend, and cause to be bought, great numbers. *Here now is a suggestion*—what enterprising publisher will try it in this JOURNAL? LIBER.

A GOOD WORD FOR OUR (VERMONT) HOMŒOPATHISTS.—It is at least creditable to our homœopathists, in *Vermont*, that whilst those in Massachusetts have been absurdly fighting for affiliation with the State *Medical* Society, our own Hahnemannists are in entire accord with ourselves in our repugnance to *professional* association with each other.

Vermont, July 3d, 1873. GALEN.

MORTALITY IN MASSACHUSETTS.—*Deaths in twelve Cities and Towns for the week ending July 5*, 1873.

Boston, 163—Charlestown, 11—Worcester, 15—Chelsea, 6—Cambridge, 15—Lawrence, 17—Springfield, 5—Newburyport, 3—Somerville, 9—Fall River, 16—Haverhill, 10—Holyoke, 12. Total, 282.

Prevalent Diseases.—Consumption, 44—cholera infantum, 41—pneumonia, 16—scarlet fever, 13.

Four deaths from smallpox occurred in Holyoke and two in Somerville. The deaths from scarlet fever were in Boston and Fall River.

GEORGE DERBY, M.D.,
Secretary of the State Board of Health.

DEATHS IN BOSTON for the week ending Saturday, July 12th, 128. Males, 69; females, 59. Accident, 4—apoplexy, 1—asthma, 1—bronchitis, 2—inflammation of the brain, 4—congestion of the brain, 2—disease of the brain, 1—burned, 1—cancer, 2—cerebro-spinal meningitis, 3—cholera infantum, 23—consumption, 20—convulsions, 2—diarrhœa, 1—dropsy, 1—dropsy of the brain, 1—drowned, 1—diphtheria, 1—erysipelas, 2—scarlet fever, 10—typhoid fever, 3—bilous fever, 1—disease of the heart, 4—homicide, 1—intemperance, 1—jaundice, 1—disease of the kidneys, 5—disease of the liver, 1—congestion of the lungs, 1—inflammation of the lungs, 3—malformation, 1—marasmus, 6—measles, 3—old age, 2—premature birth, 1—puerperal disease, 2—scrofula, 1—disease of the spine, 1—suicide, 2—ulcers, 1—unknown, 4.

Under 5 years of age, 61—between 5 and 20 years, 20—between 20 and 40 years, 21—between 40 and 60 years, 19—over 60 years, 7. Born in the United States, 98—Ireland, 21—other places, 9.